H1044
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12.5.1
10/25/11

To:
Cc:
Bcc:
Subject: Draft Final EW ERA and Response tp EPAs Comments

From: Dan Berlin <dberlin@anchorqea.com>
To: Ravi Sanga/R10/USEPA/US@EPA
Cc: Debra Williston - Home (b) (6), Debra Williston - Work <debra.williston@kingcounty.gov>, Doug Hotchkiss <hotchkiss.d@portseattle.org>, Gary Pascoe (b) (6), Jeff Stern <jeff.stern@kingcounty.gov>, Kirsten Payne <kpayne@anchorqea.com>, Kym Takasaki <takasaki.k@portseattle.org>, Pete Rude <pete.rude@seattle.gov>, Scott Becker <sbecker@integral-corp.com>, Susan McGroddy <susanm@windwardenv.com>, Tom Wang <twang@anchorqea.com>
Date: 10/25/2011 09:43 PM
Subject: Draft Final EW ERA

Ravi,
The draft final ERA has been posted to the East Waterway website. It has been posted to both the EPA version and stakeholder version. The document has redline text addressing EPA comments. In addition, two rockfish samples that were accidently omitted from the draft ERA have been added. The additional rockfish resulted in changes to Maps 4-1 and 6-12. The complete map folio has been posted for your review. The only attachments that have been amended are Attachments 2,3 and 5 and all changes to the attachments are in redline as well. Please let me know if you have any questions or concerns.

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Anchor QEA's Seattle office has moved. Please update your records to reflect our new address.

Response to EPA Comments:

Comment	Page	Section	Comment
1.	General	General	Please add a rationale for not characterizing dioxin as a COC. More discussion with EPA is needed regarding this issue. **Response:** Text and footnote was added to Section 2.5.1.1 (third paragraph).
2.	General	General	Please add a detailed justification for identifying risk drivers in the Eco- Risk assessment. Please remove the risk driver column from the tables in Section A.7. **Response:** Risk drivers have been retained in Section A.7. Additional language has been added to Section 7 detailing the importance of both risk drivers and COCs in the SRI and FS. The identification of risk drivers in the EW ERA is consistent with the identification of risk drivers in the LDW ERA.
3.	General	General	Please revise the Benthic TRV for TBT using data from Gibbs et al., (1988) which is the residue associated with late stage imposex resulting in complete reproductive impairment in female *Nucella* (dogwinkle). The resulting LOAEL,0.12 mg/kg wet, is appropriate in light of the following considerations: 1. Inclusion of sterilization due to imposex is a valid consideration for EW since prosobranch gastropods have been observed in the waterway (1988 PTI study and during recent benthic sampling for Superfund). 2. The TRV value, itself, and inclusion of the imposex-mediated sterility endpoint is consistent with other regional Superfund risk assessments for TBT. 3. Recent research shows that gastropods are not the only aquatic organisms whose reproduction is affected by the endocrine disrupting effects of TBT. TBT appears to impair reproduction of fish, amphipods and other organisms at tissue concentrations approaching (if not lower) than those causing sterilization in gastropods. Please make a note of this in Section A.7.1. **Response:** Comment addressed. A footnote was added to Section A.2.5.1.2; and the following tables or sections were revised: Table A.2-18; Section A.3.2.2.1; Table A.3-9; Section A.6.1.1.2; Section 6.1.1.5; Table 6-10; Section A.7.1; Table A.7-1; Section 8.2; and Table 8-1.
4.	General	General	Retain the growth-based fish TRV for TBT (0.290 ppm wet) from Triebskorn et al., 1994. Acknowledge in the text that this TRV likely overestimates the LOAEL citing the growth-based LOAEL of 0.159 ppm wet in Japanese flounder carcass from Shimasaki et al (2003) and that zebrafish masculinization has been observed by Santos et al., (2006) at 0.047 ppm wet. Please note that exclusion of vicera in the analysis performed by Shimakaki et al (2003) would have a negligible effect on the whole body concentration given that the mass of the vicera is so small relative to the rest of the carcass. EPA can provide detailed calculations supporting this statement upon request. In addition, 1. EPA's decision on risk drivers for a sensitive/threatened and endangered species such as rockfish must reflect the weight-of-evidence from the scientific literature and must consider both population and individual fish residues. 2. The fact that the EPC for Rockfish (0.22 ppm wet) exceeds both the LOAEL from Shimasaki et al. (2003) and that from Santos et al. (2006) and that 3 of the individual rock fish residues exceed the Triebskorn-based TRV value would indicate that TBT must be carried forward as a risk driver for Rockfish. Please make note of this in section A.7.2 **Response:** The requested changes to the TBT TRV were made. The brown rockfish is not listed as threatened or endangered and therefore we believe that the EPC is the appropriate measure of effects on the population and the HQs calculated for the three individual fish were 1.1, 1.1 and 1.8. Therefore, while TBT is a COC for rockfish, it has not been identified as a risk driver for rockfish. The Shimaski et al. (2003) and Santos et al (2006) studies are both discussed in the uncertainty section.

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5.	General	General	More discussion is needed regarding how \ toxicity from mixtures is being assessed. If they are not directly assessed as part of this RI/FS process, then at a minimum, please add a discussion about toxicity often being additive when chemicals co-occur. This is especially relevant for this site where PCBs and TBT occur together. Toxicity can occur even when individual contaminant concentrations are below their no-effect concentrations (e.g. Nakayama et al. 2008). Please add text to the uncertainty sections and to each section and table where risk uncertainty is described to explain the decision uncertainty surrounding omitting chemical mixtures. **Response:** Text was added to uncertainty sections for benthic invertebrates (tissue-residue approach); crab (tissue residue approach), and for all fish and wildlife ROCs. It must be noted that chemical mixtures can have additive or antagonistic effects (i.e. Santos et al. 2006 where the presence of estrogenic compounds in addition to TBT resulted in no effect compared to the TBT only exposure).
6.	General	General	Please balance the uncertainty discussions. Most uncertainty explanations only describe uncertainties that potentially overestimate risk. These discussions need to also address factors that may result in risk being underestimated. Some of these factors include: not all endpoints were considered (such as behavior and other endpoints that can have effects on reproductive success, survival, and growth); test species or life-stages used in the selected study were less sensitive than receptors of concern; and dosing intervals were often large and therefore thresholds were not necessarily captured due to experimental design. Please ensure that the uncertainty descriptions are scientifically accurate and balanced. **Response:** Comment addressed. Text was added to uncertainty sections for benthic invertebrates (tissue-residue approach); crab (tissue residue approach), and for all fish and wildlife ROCs.
7.	General	General	Many of the assumptions used in this ERA described as "conservative" are actually not conservative. They may be reasonable estimates but not conservative. See for example the "Site Use Factor" comment on section 5.1.2.4. This leads to the ERA claiming that the resulting risk estimates are conservative estimates when in fact they are not. EPA is concerned that this in turn will lead to the RI/FS proposing cleanup options and describing them as conservative when in fact they are not, so then when it comes time to select a remedy, there will be pressure not to be overly "conservative" and to pick a more "reasonable" alternative that will actually be less protective than is warranted. Please change the language throughout this document accordingly. **Response:** Comment addressed. Text was revised in the following sections: A.3.4.1.3; A.4.2.2.2; A.5.1.1; A.5.1.2.4; and A.6.1.1.3.
8.	General	General	EPA is concerned that there are multiple locations in the document where imprecise wording might lead to misinterpretations of risk by implying that a claim that relates only to one contaminant, receptor, or pathway applies to others as well. See for example the comment focused on page 249 (shells). Please check to ensure that all claims made in the ERA are clearly stated with the appropriate qualifications and caveats. **Response:** Comment addressed. The text on page 249 was revised and the document was checked
9.	General	General	Since whole-body tissue data is sparse for fish in East Waterway, we recommend supplementing it with fillet data converted to whole body values on a percent lipid basis. **Response:** Given that the fillet data is from same trawls as whole-body data and the high degree of uncertainty involved in converting it to whole body concentrations, EPA does not agree that the proposed approach would improve the data set. Therefore, this comment has been withdrawn by EPA (meeting on July 21, 2011).
10.	General	General	The ERA states that dioxins, organometals, cobalt, molybdenum, and vanadium were not considered as COPCs in sediment for benthics because these chemicals do not have SMS or DMMP criteria. It is inappropriate to screen out contaminants without regulatory criteria to demonstrate that the contaminant poses no risk. Please find alternative numerical guidelines for these chemicals in sediments, or else retain them as potentially contributing to ecological risk with the EW. **Response:** A search was conducted for sediment toxicity values for these compounds based on benthic invertebrate toxicity and none were found. The uncertainty associated with these chemicals is discussed in the uncertainty section.

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11.	General	General	The field notes from the October 2008 survey list numerous snails found, but do not specify whether they are adult or juvenile. Therefore it must not be specified that only juvenile snails were found. Also, because snails typically are found on hard substrate, van Veen sampler sediment grabs may not be a particularly effective method for collecting snails, and the benthic sledge may also be ineffective at collecting snails if it is used only in deep water. Please add a discussion of the potential effect of TBT to snails. Please also add a discussion of potential adverse effects on gastropod species in EW as a result of TBT exposure. **Response:** Comment addressed. The benthic invertebrate tissue TBT TRV is now based on imposex in gastropods and the text has been revised with regard to the juvenile snails.
12.	General	General	Please evaluate copper in terms of olfactory impact on juvenile salmon and whether this impedes migration and therefore reproduction. **Response**: No toxicity data were identified to evaluate the impact of copper in terms of olfactory impact in marine waters.
13.	General	General	Please add more discussion regarding bioaccumulation. The ERA must include a complete discussion of direct and indirect risks through bioaccumulation. **Response**: Bioaccumulation is explicitly addressed in the tissue residue evaluation for benthic invertebrates and fish as well as the dietary assessment for wildlife. No text was revised in response to this comment.
14.	viii	TOC	The table of contents is incomplete. For example, no tables are cited beyond Table A.3-21. Please review the entire TOC and add all missing information. **Response:** Comment addressed. The table of contents is now complete.
15.	ES-3	ES.3	Effects Assessment: The executive summary states that "For COPCs without SMS criteria (i.e. total DDTs), guidelines from the Washington State Dredged Material Management Program (DMMP) were used." DMMP guidelines for dioxins and TBT exist, but were not used. These chemicals must be included in the ERA. Please note that here. **Response:** The DMMP guidelines for TBT and dioxins are not based on whole sediment toxicity data and therefore were not used to characterize risks in the ERA.
16.	ES-6	ES.4.5	Table ES-1: Dredged Material Management Program (DMMP) guidelines are available for TBT and dioxins. Please revise. **Response:** The DMMP guidelines for TBT and dioxins are not based on whole sediment toxicity data and therefore were not used to characterize risks in the ERA.
17.	3	A.2.1.1	Please add language regarding the Tribes current and future use. **Response:** Comment addressed. Language was added to Section A.2.1.1.
18.	6	A.2.2.1	Table 2-1: An explanation of the abbreviation SE (in the killer whale status column) must be added to footnote a. Please clarify whether this abbreviation represents a state endangered species or something else. **Response:** Comment addressed. Abbreviation of SE (state endangered) was added to footnote a.
19.	22	A.2.2.4	Please add language regarding the Pacific Flyway and importance of water bodies to migratory birds for feeding and resting. **Response:** Comment addressed.
20.	25	A.2.2.4.4	Please add language regarding the Pacific Flyway and importance of water bodies to migratory birds for feeding and resting. **Response:** Comment addressed.
21.	33	A.2.3.2.3	Top of page, delete "appear to be" from "rockfish are appear to be." **Response:** Comment addressed.

22.	34	A.2.3.3	Last paragraph of this section states that "pigeon guillemot are expected to have higher exposure than benthivorous birds because a higher proportion of its diet is likely to be obtained from the EW." If the authors believe this to be true, it must be stated and not speculated and then referenced to hold up their argument. For example, 2.3.3.2 must include data on site-specific sightings of pigeon guillemot compared to other benthivorous species, and likewise there must be a citation for the claim that "bufflehead and goldeneye do not breed in the vicinity of the EW" on the bottom of page 35. **Response:** Comment addressed.Two footnotes were added to Section A.2.3.3.2
23.	37	A.2.3.3.4	Harbor Seal: "there is more anecdotal evidence of the presence of harbor seals in the EW than of [sea lions and harbor porpoises], so it is assumed harbor seals feed there more often." This assumption must be contextualized based on feeding behaviors (e.g. if harbor seals need to come up for air more frequently than other species when feeding, it is possible they may be observed more often even if the other species actually spend more time there). Please add the appropriate discussion. **Response:** The dive times for sea lions and harbor seals appear to be similar with average dive times of 2-6 min for harbor seals and 3 min for sea lions (references below). Therefore, no changes were made to the text. References: Diving patterns of California sea lions, Zalophus californianus Steven D. Feldkamp, Robert L. DeLong, George A. Antonelis Pharmacological blockade of the dive response: effects on heart rate and diving behaviour in the harbour seal (Phoca vitulina) Nicole M. Elliott1, Russel D. Andrews1,* and David R. Jones
24.	37	A.2.3.3.4	Please revise the last line of the paragraph on harbor seal to read "therefore may receive EW contaminants through their diet." **Response:** Revision was not made based on a discussion with EPA on July 21, 2011. It was not clear how the additional requested text would add to the existing text and therefore EPA agreed no changes were needed.
25.	42	A.2.4.1.1	Table 2-10: Please footnote a rationale or provide additional rationale in table cell as to why EW Benthic Tissue 08 and 09 was excluded due to compositing. Composite samples were used in some other cases for this ERA. Please clarify the type of compositing done and what ramifications this may have on exclusion. **Response:** Comment addressed. A footnote was added to Table 2-10.
26.	43	A.2.4.1.2	Tissue Chemistry: Although this was described in the EISR, please provide a brief explanation why 1995 was selected as a cutoff date for using tissue chemistry data. **Response:** The 1995 cutoff was selected to represent the most current baseline site conditions and to include all tissue datasets for which there is sufficient documentation available to support a QA review.
27.	47	A.2.4.1.3	The metals dataset were combined because ranges were comparable. Please mention if there were any statistical tests performed to confirm that these datasets warrant combining. **Response:** The combined dataset was described in the ERA technical memo ((Windward 2010). The data were not evaluated statistically. Additional text was added to address this comment.

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28.	48	A.2.4.1.5	Text states that bioassay results were not included if more recent chemistry data was available from the same location. More explanation is needed in this section regarding the exclusion of older bioassay data. There are two key reasons to use the bioassay data. First, given the spatial variability, more data is better here. The bioassay data could be used in combination with the chemistry data, which would essentially result in smaller Thiessen polygons. Second, and most importantly, the bioassay data could represent the combined effect of *all* contaminants, while the chemistry data only represents the contaminants that were analyzed for. As an example, consider the case where a bioassay identified a sediment sample as toxic, but the subsequent chemistry sample comes up clean. Some chemicals such as dioxins (& TBT?) were not analyzed in all chemistry samples, so the "clean" chemistry result might be erroneous if the particular chemical(s) causing the toxic effect were not analyzed. **Response:**The bioassay data must be evaluated with the chemistry data that was collected at the same time. For example, several bioassay samples were post-dredge monitoring samples. The more recent chemistry were sufficiently different from the original results and therefore the older bioassay data would likely not be consistent with conditions represented by more current chemistry results. In the cases where the more recent data does not include all the analytes in the original sample, such as TBT, then, the original chemistry result is retained for that chemical. No changes were made to the text in response to this comment.
29.	54	A.2.5	Table 2-15: Top row, right column: Using the methods in this risk assessment, chemicals for which there is no SMS criterion or DMMP guideline (including cobalt, molybdenum, vanadium, the three organometals, and dioxins/furans) are not considered COPCs, because the COPC list is established based on exceedances of these criteria and guidelines. The Uncertainty Analysis in section 6.1.1.1 (page 229) states that despite the lack of criteria for these chemicals, the locations with the highest potential for adverse effects on the benthic community were likely adequately identified "because criteria and guidelines are available for chemicals within most of the chemical groups that are generally considered in CERCLA investigations." This is *not* an adequate method for identifying *locations* posing risk. If there was reason to believe that the chemicals that do not have numeric criteria were co-located with the others, then there would be more confidence that the EcoRisk Assessment was adequately identifying the contaminated locations. Please add information on EW history that would indicate that, for example, if organometals and dioxins are co-located with PCBs. Additionally, the EcoRisk Assessment needs to not only identify locations that pose risk, but also quantify the risk (at least as a rough estimate). **Response:** . The last paragraph in the subsection entitled "Uncertainties Associated with Sediment Risk Estimates" was deleted and text was added regarding the general uncertainty in evaluating risk for chemicals without SMS, DMMP, or relevant toxicity information. Literature on the toxic effects of dioxins and the other chemicals for which we don't have SMS criteria must be consulted to develop criteria specific to the EW. Page 230 describes other lines of evidence that were used to assess risk from TBT, cobalt, and molybdenum. Similar efforts must be done for dioxins/furans and the others. These contaminants must be dealt with in some way, especially since there has been public concern over dioxin in the Duwamish area and concentrations are also elevated in EW. More discussion is needed between the EPA, EWG and stakeholders on this issue. **Response:** A search was conducted to identify sediment criteria based on benthic invertebrate toxicity the chemicals for which we had no criteria and no suitable values were found. Text and footnote was added to Section 2.5.1.1 (third paragraph). This text explains that dioxin was evaluated for other ecological receptors.
30.	54	A.2.5	Table 2-15: 2nd row, tissue: Section 2.5.1.2 describes the rationale for selecting PCBs, TBT, and Hg, but does not include the rationale for excluding other contaminants. Also, page 1 of Attachment 2 must list criteria for benthic invertebrate tissue as it lists criteria for other receptors. **Response:** Comment addressed in Section 2.5.1.2. The tissue residue approach was required for TBT due to the lack of a sediment value. In addition, a tissue residue evaluation was conducted for PCBs and Hg as a supplementary line of evidence to the primary evaluation based on sediment due to fact that these chemicals had the highest number of SMS exceedances in sediment. Attachment 2 has been revised.

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31.	54	A.2.5	Table 2-15: 3rd row, porewater: please explain/add the rationale for excluding other chemicals from porewater analysis. **Response:** Comment addressed. A footnote was added to Table 2-15.
32.	56	A.2.5	Definition of NOAEL is incorrect. Please change to "maximum level *at* which adverse effects have not been observed" (not "*below* which"). **Response:** Comment addressed. Text was revised.
33.	58	A.2.5.1.1	Table 2-16: It is very unusual that DDD and DDT were detected but not DDE. Please explain why DDD and DDT were detected, but not DDE. **Response:** Comment addressed. There was a typographical error in footnote c of Table 2-16. The detected components of total DDT were 4,4'-DDD and 4,4'-DDE. The footnote has been corrected.
34.	62	A.2.5.1.2	PAHs, DDTs, and other bioaccumulative contaminants can also accumulate in benthic invertebrate tissues and must also be considered as COIs here unless a rationale can be provided otherwise. **Response:** The tissue residue approach was required for TBT due to the lack of a sediment value. In addition, a tissue residue evaluation was conducted for PCBs and Hg as a supplementary line of evidence to the primary evaluation based on sediment due to fact that these chemicals had the highest number of SMS exceedances in sediment. PAHs were evaluated based on sediment SMS values. DDTs were infrequently detected in sediment and never measured in benthic invertebrate tissues.
35.	62	A.2.5.1.2	Dredged Material Management Program (DMMP) guidelines are available for TBT and dioxins. Please add. **Response:** The DMMP guidelines for TBT and dioxins are not based on whole sediment toxicity data and therefore were not used to characterize risks in the ERA.
36.	63	A.2.5.1.2	*"All selected TRVs were based on laboratory toxicological studies. Studies using field-collected data (i.e., field-collected organisms) were not considered acceptable. Field studies were not used to derive TRVs because adverse effects observed in organisms during field studies may be attributed to the presence of multiple chemicals and/or other uncontrolled environmental factors, rather than to a single test chemical."* Since organisms in EW are exposed to chemical mixtures, please explain in the uncertainty section why only lab studies with single chemical exposures are being used as realistic predictors of effects of exposure to mixtures. Many studies have observed that effects from contaminant mixtures are additive, for example, PCBs and TBT effects-thresholds are lower when both substances are present. Laboratory studies use organisms that are tolerant to stress and have short life spans and therefore do not represent many field organisms' sensitivity or contaminant exposure duration. The underlying uncertainties for both laboratory and field studies must be clearly and adequately discussed. **Response:** Comment addressed. Text was added to the uncertainty analysis in Section A.6.1.1.2.
37.	64	A.2.5.1.2	Table 2-18: Please explain why a NOAEL wasn't calculated for mercury for the slipper limpet based on the LOAEL and uncertainty factors. **Response:** A NOAEL from a different study (Tessier 1996) using the same endpoint (growth) was used, as shown in Table 2-18. The TRV approach uses existing NOAELs when available and below the lowest LOAEL rather than calculating NOAELs using uncertainty factors and LOAELs.

38.	72	A.2.5.1.5	Footnote 7 claims that "chemical concentrations in hepatopancreas tissue are likely to provide a conservative estimate of internal organs in general because the hepatopancreas constitutes the great majority of organ mass and has a relatively high lipid content compared to other organs." This may be true for lipophilic organic chemicals like PCBs, but please explain if it applies to metals as well. Please provide a citation showing that As, Cd, Cu, and Zn preferentially partition to high-lipid-content organs. Also, if the measured hepatopancreas values are to be compared with *whole-body* NOAELs as described in the text at the top of this page, then the footnote is somewhat irrelevant when it talks about whether hepatopancreas conservatively estimate *other internal organs*, because concentrations on the shell can be very significant. Please change accordingly. **Response:** Footnote was deleted because it was not relevant to the COPC screen.
39.	79	A.2.5.2.1	Table 2-30: For mercury, replace the LOAEL from Matta et al. (2001) with the corrected LOAEL of 0.39 mg/kg wet reported in Hammerschmidt et al. (2002) **Response:** Comment was addressed and changes were made to the following tables or sections: Table A.2-30; Section A.4.2.1.1; Table A.4-11; Section A.6.2.3.1; Table A.6-28; Section A.6.2.3.3; Table A.6-33; Section A.7.2; Table A.7-2; Section A.8-2; and Table A.8-1.
40.	79	A.2.5.2.1	Table 2-30: For total PCBs, using survival as an effect endpoint for risk for a protected salmon species is not appropriate. The narcosis mode of action for PCBs is orders of magnitude higher than for other relevant sub-lethal modes of action that cause growth and reproductive effects. Therefore the number used from this paper needs to be adjusted to a sublethal effect concentration (e.g. factor of 10 lower). In addition, Hansen et al. (1971) uses a species that is less sensitive than salmon (about 8 fold less sensitive than a salmonid according to EPA ICIS website). Adjustment should also be made for this large difference in species sensitivity (e.g. reduce by a factor of 8). However, as stated in the comment for page 168 the simplest approach would be to use the lowest TRV from the Hugla and Thome (1999) paper for this receptor, and state that while the salmon do not reproduce in East Waterway, this sublethal endpoint is a surrogate for other sublethal endpoints such as growth. **Response:** Comment was withdrawn by EPA based on revisions made to text (see comment #44) discussing the PCB TRV relating to juvenile Chinook salmon growth (meeting on July 21, 2011).
41.	79	A.2.5.2.1	Table 2-30: The NOAEL and LOAEL for DDT were not correctly pulled from Allison et al. 1964 and definitely must not be the same number. Given the analytical method used to quantify the DDTs in the Allison et al. (1964) paper, the high DDT concentrations in the control organisms, and the lethal endpoint, a more protective low-effect concentration must be used from this paper. The LOAEL from this paper is 1.1 ppm and the NOAEL is 0.8 ppm. The possibility that these effect concentrations are elevated above actual effect concentrations also must be discussed in the uncertainty section. **Response:** Comment addressed. Based on a subsequent comment from EPA in an email dated August 16, 2011, the LOAEL TRV of 1.8 mg/kg ww was retained and the NOAEL TRV was revised to 0.9 mg/kg ww. Revisions were made to Tables A.2-30 and A.2-31.
42.	79	A.2.5.2.1	Table 2-30: Impacts to early life stages, which are more sensitive, may occur for TRVs based on adult life stages. Please impartially discuss this uncertainty. **Response:** Comment addressed. Text was added to the sixth paragraph of Section 2.5.2.1 (i.e., the paragraph immediately preceding Table A.2-29).
43.	80	A.2.5.2.1	Table 2-31: Please explain why the TEQs are bolded when they are not selected as a tissue COPC. Please clarify or correct. **Response:** Comment addressed. The bold and underline was removed from the total TEQ maximum chemical concentration in Table 2-31.

44.	81	A.2.5.2.1	The study by Mauck et al. 1978 measured reduced growth in brook trout at 48 days. No tissue residues were available for that time frame. Tissue residues in 7 day old fry were 3.2 ppm and in 118 day old fry were 71 ppm. There is no way to know the residue that caused the reduced growth, but using this study to say no growth effects have been measured up to 31 ppm is incorrect. Reduced growth during larval development can have a significant impact on survival. Please correct. **Response:** Text describing the NOAEL TRV for PCBs in salmon (Section 2.5.2.1) of was revised to reflect the range of concentrations in Mauck et al. 1978.
45.	82	A.2.5.2.2	Direct contact, sediment ingestion, and prey ingestion are all separate pathways by which fish may be exposed to contaminants. Therefore, on page 82, COIs must include chemicals that meet *either* of the criteria, not necessarily all. The weighted average approach on p. 86 incorporates sediment ingestion but not direct contact, and it does not include chemicals that were only in prey or sediment but not both. The weighted average must be expanded to include a term for direct contact and must be applied to all chemicals that were detected either in prey or in sediment. Alternatively, a separate section (2.5.2.4) could be written to address direct contact (and the current 2.5.2.4 Summary section would be then shifted to 2.5.2.5). **Response:** Direct contact pathways with sediment are normally not evaluated for fish. No text was changed in response to this comment.
46.	89	A.2.5.3	Wildlife COIs. Here and elsewhere where the criterion is "detection in at least 5% of sediment samples", none of the samples were analyzed for the entire suite of chemicals, so if only 25% of samples were analyzed for dioxin, for instance, please clarify if this criterion is 5% of the total number of sampling locations or 5% of the number of samples analyzed for dioxin. Presumably it is the latter, but please clarify. **Response:** Comment addressed. Text was revised to indicate the chemical had to be detected in at least 5% of the samples in which it was analyzed. Text was revised in Tables A.2-15 and A.2-16, in the first paragraphs of Sections A.2.5.1.5, A.2.5.2.1, A.2.5.2.2, A.2.5.3, and in the sentence immediately following Table 2-17.
47.	89	A.2.5.3	Sediment contact and incidental ingestion pathways need to be discussed with regard to COPC's. **Response:** Comment addressed. Text was added to the paragraph after Table A.2-38 and a footnote was added in the paragraph below Equation 2-2.
48.	98	A.2.5.3	Wildlife COPCs are diet-based and don't incorporate direct contact with sediment or incidental ingestion of sediment. The rationale given on page 98 is that the approach is "conservative" because it uses the maximum tissue concentration in the prey to represent dietary exposure. However, sediment ingestion and direct contact are separate exposure pathways that *add* to the exposure from prey in the diet, thus omitting them is not conservative. They must be roughly estimated in an ERA. **Response:** Sediment is a small portion of the diet and when the sediment was added to each of the ROC's diets at a rate of 2% of the food ingestion rate using the maximum sediment concentration in the screening calculations, it did not change the results of the COPC screen. There are no methods to quantify exposure of wildlife from direct contact with sediment.
49.	111	A.2.6	Fur and/or feathers are not an acceptable rationale to consider exposure to sediment and water insignificant. For birds the skin on their faces is bare of feathers and very thin and their legs and feet do not have feathers (there are also areas under their wings that have few or no feathers). Mammals lack fur on areas of their face and feet and the fur under their "arms" and on the insides of their legs is typically thin. Please add text to reflect this. **Response:** Comment addressed. Based on a discussion with EPA on July 21, 2011, additional text was added to the last paragraph of this section.
50.	111	A.2.7	Please add a more detailed discussion of potential endpoints (behavior, etc.) as it can result in reduced survival, growth and reproduction. **Response:** Comment addressed. A sentence describing the use of biomarker, behavioral and histological endpoints was added to the first paragraph in Section A.2.7.

51.	130	A.3.2.2.1	Section 3.2.2.1 on TBT incorrectly states that "the imposex observed in gastropod snails has not been observed in other species at those low tissue concentrations and may occur by a unique mechanism specific to gastropods (Meador and Rice 2001)." More recent work by Jim Meador has clarified that TBT is a very potent endocrine disruptor whose effects at very low tissue concentrations are *not* restricted to snails (Meador 2011). Therefore, other benthic invertebrates should be evaluated for reproductive risk due to TBT. Please add a discussion to reflect this. **Response:** Comment addressed. The TBT TRV has been revised. The new TRV is based on reproductive risk due to imposex. Meador 2011 notes that recent research has shown reproductive effects in fish at concentrations similar to concentrations associated with effects in gastropods and speculates that "As more research *is conducted, it will likely become evident that these very low tissue concentrations are able to cause adverse effects in a variety of taxa*" (Meador 2011, pg 279). The fish tissue studies are addressed in the discussion of the fish tissue TBT TRV and are not discussed in the benthic section.
52.	137	A.3.3.1	(Tissue-residue exposure assessment): Please provide specific method used to calculate whole-body crab concentrations from the edible meat and hepatopancreas data. Cite the relative weights/contributions of each tissue type to the total mass of crab and source of this information. **Response:** Comment addressed. A footnote was added to Section A.3.3.1 describing the method.
53.	137	A.3.3.1	The last sentence of the paragraph is confusing. Please delete. As shown in the table, nine samples were available for each chemical, so this issue with using the max rather than the 95% UCL on the mean never occurred. The same comment applies to page 148 where in every case there were at least 6 samples. In general, the ERA will be most clear and readable if it describes what was actually done, not what would have been done under different circumstances. **Response:** Comment addressed. This sentence was deleted in the two instances where it was not needed: in Sections A.3.3.1 and A.4.1.1.
54.	137	A.3.3.2	Define "relatively mobile" for crabs. Please explain if there is evidence to show that a crab will move around the entire East Waterway or if it is more likely to stay near shore. For Cd, there is a difference of more than an order of magnitude between the individual maximum concentration and the 95% UCL. For Hg there is a difference of about a factor of 3. Please revise this section accordingly. **Response:** Comment addressed. A footnote on the mobility of crabs was added.
55.	140	A.3.4.1.2	(Cadmium): Please include the citation of the Vernberg et al. study in the text discussing why it wasn't used to set the TRV for cadmium. **Response**: Comment addressed. The Vernberg et al citation was added.
56.	141	A.3.4.1.3	Copper can cause hyperactivity in fish as well as interfere with the ability of fish to respond positively to L-alanine, an important constituent of prey odors (Contaminant Hazard Review – Copper). Please add this information. **Response:** Comment addressed. This comment is in the crab section but refers to fish; therefore this information was added to Section A.4.2.2.4 for fish.
57.	142	A.3.4.1.4	(Zinc): LOAEL TRV for Zinc (35.2 ppm ww) is the same as that used in the LDW ERA but is attributed to a different reference (Mirenda 1986 in LDW ERA vs Ahsanullah et al. 1981 in EWW ERA). Please resolve discrepancy. **Response:** Comment addressed. The Ahsanullah reference was replaced with the correct Mirenda reference.
58.	147	A.4.1.1	Studies have shown that juvenile Chinook bio-accumulate substantial levels of toxic chemicals. It can't be assumed that early levels of exposure are not going to potentially affect health and survival. An article by James Nagler et al. (*High Incidence of a Male-Specific Genetic Marker in Phenotypic Female Chinook Salmon from the Columbia River*) states that the phenotype of male salmonids can be changed to that of the female by exposure to estrogenic steroids during an estrogen sensitive window that occurs from the time of hatching and extends beyond the time when the fish begin to feed exogenously. Some of these environmental estrogens consist of but are not limited to atrazine, carbofuran, lindane, methyl parathion, and dieldrin. **Response:** The information in the cited article is not sufficient to develop a TRV and therefore was not used in the ERA.

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59.	148	A.4.1.1	In every case there were at least 6 samples for each chemical, so this issue with using the max rather than the 95% UCL on the mean never occurred. In general, the ERA will be most clear and readable if it describes what was actually done, not what would have been done under different circumstances. Please revise accordingly. **Response:** Comment addressed. This sentence about using the maximum if there were less than 6 samples was deleted in the two instances where it was not needed: in Sections A.3.3.1 and A.4.1.1.
60.	150	A.4.1.2.1	Equation 4-2 appears to be mislabeled (i.e. there is no equation 4-1). Please correct. **Response:** Comment addressed. Equation 4-2 was relabeled as Equation 4-1.
61.	161	A.4.2.1.1	Table 4-11: The LOAEL derived from Hammerschmidt et al. (2002) must be 0.39 mg/kg rather than 0.56 mg/kg. The LOAEL from this study will be the basis of the TRV for mercury in fish. The lower value reflects whole body residues in males with significantly reduced spawning after exposure as juveniles. The study's authors determined that this early life stage exposure was causal with regard to reduction of spawning success in the fathead minnows tested. The higher residue value (0.56 mg/kg ww) reflects the body burden in individuals exposed both as juveniles and as adults. **Response:** This comment was addressed. Please refer to response to comment 39 for specific sections and tables that were revised. There is also a discrepancy in Webber and Haines (2003) between the whole body residue for the high mercury treatment noted in the abstract (518 ng/g wet) and that reported for the same treatment in the body of the paper (536 ng/g wet in Table 1). **Response:** Comment addressed. A footnote was added to Table 4-11 indicating that the abstract presented a different value than the body of the paper.

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62.	163	A.4.2.1.2	Table A.4-12 (TBT TRVs for tissue residue effects in fish): Revise data given in Table A.4-12 (and Risk Conclusion Tables) for the following studies: The LOAEL derived from Triebskorn et al. (1994) should be revised to reflect a calculated whole-body concentration using residues reported for both "head" and "rest body". The concentration reported in Table A.4-12 (0.27 mg/kg ww) reflects a "rest body" TBTO concentration. Whole-body concentration calculated assuming a head to body mass ratio of 1:4 would be slightly higher at **0.29 mg/kg** ww. This value sets the TRV for TBT in fish. **Response:** Comment addressed. Requested revision was made to Table A.4-12 and risk conclusion tables. The LOAEL reported for Shimasaki et al., (2006) should be revised to 0.73 – 1.37 mg/kg ww. The basis of this revision is the egg-to-adult conversion factor derived for Japanese medaka from Nirmala et al. (1999). The Nirmala study reported 2,390 ng/g TBT in female fish and 279 ng/g TBT in their unfertilized/unhatched eggs (page 718). This corresponds to an egg-to-adult conversion factor of 8.6 (rather than 9.2 as reported in footnotes b and c of Table A.4-12). This agrees with the egg-to-adult conversion factor (8.57) derived from the same study and used in the LDW ERA. Please revise the conversion factor reported in Table A.4-12 (footnotes b and c) and in the text (page 164) to be 8.6. **Response:** Comment was addressed. The LOAEL reported for Nakayama et al. (2005) must be revised to 1.05 mg/kg ww based on use of the egg-to-adult conversion factor of 8.6 (rather than 9.2 as reported in footnotes b and c of Table A.4-12). **Response:** Comment was addressed. Add discussion of the TBT partitioning results of Shim et al., (2002) and Shimasaki et al (2008). Both studies found that TBT preferentially partitions into blood serum. Shim's paper is particularly relevant since they studied the same species (Japanese flounder) as was tested in Shimasaki et al. (2003) study. **Response:** The TBT partitioning information is interesting but it does not seem necessary for the ERA. The tissue residue concentrations and TRV are based on whole body concentrations and the partitioning behavior of other contaminants is not discussed. No changes were made based on this comment.
63.	164	Section A.4.2.1.2	Page 164, Section A.4.2.1.2, (TBT TRVs for tissue residue effects in fish): Please clarify how the species/exposures used by Nirmala et al (1999) were different than those used by Nakayama et al (2005) as stated in the text and footnote #18 (page 164). **Response:** Comment addressed. Text was in error and was deleted.
64.	164, & 165	Section A.4.2.1.3	(PCBs): The Fisher et al. 1994 reference does not appear in Table A.4-13. Please add it to table. **Response:** Comment addressed. The Fisher et al. 1994 reference was added to Table A.4-13.

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65.	168	A.4.2.1.3	2nd paragraph. This comment also relates to Tables 6-27 and 6-33 footnotes. One of the strengths of the Hugla & Thome (1999) study was its long duration and measuring effects for an extended period after PCB exposure. The explanation given in this paragraph for high uncertainty for the fecundity endpoint must be changed. It must not state that the fecundity was not dose-responsive, as evaluating dose-responsiveness for the fecundity endpoint in this paper is complicated given the temporal component of the study. *The fecundity endpoint is challenging to represent across spawning seasons. Fish in the high dose did not spawn the first year so* there were no fecundity data available for averaging. The fecundity endpoint only compared numbers of eggs produced when spawning occurred (i.e. the authors did not average the 0 eggs produced in the high dose fish the first year with subsequent spawnings). Fecundity is time-averaged for the low-dose treatment (spawning immediately following exposure when tissue concentrations were higher, were combined with spawnings 1 year later when tissue concentrations were lower as evidenced by lower PCB concentration in liver). In the high dose treatment fecundity is only measured 1 year after exposure when PCB concentrations had declined *and were probably more similar to concentrations in the low-dose treatment immediately after exposure. Please accurately represent the information from this paper* and change text in this section accordingly. **Response:** EWG recognizes EPA's concerns with the lengthy text regarding the Hugla and Thome (1999) paper. However, as part of the LDW ERA, substantial effort was made to come to an agreement between EPA and LDWG on the content of Hugla and Thome description. Since no new information has arisen since this text was finalized in the LDW ERA in 2007 the text has not been revised.
66.	168	A.4.2.1.3	3rd paragraph. Please add this important sentence copied from section 6.2.2.2 to this paragraph "Uncertainties in the Hansen et al. (1974a) study include elevated PCB concentrations of 0.52 to 0.64 mg/kg ww in control fish." Please also add to this paragraph that these concentrations in control fish are higher than reported effect concentrations and that fish can develop resistance to PCBs, as reported in recent work by Isaac Wirgin and Mark Hahn. Given that fish used in the study were already exposed to PCBs and probably resistant, the effects reported in the Hansen paper likely underestimate the observed effect concentrations. Please include a more complete description of uncertainty in all relevant areas of the document. **Response:** Text was added (Section 6.2.2.2) to note the control fish concentrations were higher than the Hugla and Thome (1999) TRV. The Wirgin et al. (2011) paper details a genetic variant observed in Atlantic tomcod from the Hudson River which results in PCB resistance in this population. It is not clear what the relevance of this result is to the control fish from the Hansen et al. (1974) study. Please use the lowest TRV from the Hugla and Thome (1999) paper and state that while the salmon do not reproduce in East Waterway, this sublethal endpoint is a surrogate for other sublethal endpoints such as growth. **Response:** Comment was withdrawn by EPA (meeting on July 21, 2011)..
67.	168	A.4.2.1.3	(PCBs), 3rd paragraph on page: Please revise the text to reflect addition of the Fisher et al. 1994 paper which represents the lowest whole-body concentration associated with extrapolating from embryo residues. **Response:** Comment addressed. The text was revised to include the discussion of Fisher et al. 1994.
68.	186	A.5.1.2.3	Please explain why juvenile weights are not used to calculate exposures as they are the more fragile portion of the population. **Response:** The use of adult body weights is consistent with the methods used in the LDW ERA and therefore allows for comparable analysis.

69.	188	A.5.1.2.4	This section averages the otter and seal male and female body weights to come up with a single number for each species. For endpoints involving reproduction in particular, it would be more logical to use the female body weight, since the majority of the studies on these endpoints involved exposure to reproducing females and their young. For the other endpoints, it would be more conservative to use the female body weight, because if concentrations pose risks to females only and not males, the population is still at risk even if half the individuals are not. **Response:** Use of female body weights rather than the average will have very little effect on the calculated HQs (i.e., the HQs would increase by 3% on average) and would not change any of the risk conclusions. Therefore, no changes were made in response to this comment. In addition, the methods are the same used in the LDW which allows for comparable analysis.
70.	189	A.5.1.2.4	Site Use Factor: Please explain how the assumptions regarding site use are "conservative". For assumption #1, it is understood that it is considered to be conservative in comparison to only getting a fraction of its food in the East Waterway and the rest in some area of lesser contamination, but it is not conservative in assuming that the harbor seal was only present on the day it was observed. Describe harbor seal behavior and explain how likely it is that a seal will be observed if it is present (i.e. if it is underwater for the majority of the observation time, it may have a low probability of being observed). Assumption #2 is that the 29 days of monitoring from December-June represent the rest of the year- explain how this is conservative. Is there reason to believe that there is actually less use in July-November than in Dec-June? Also, is there reason to believe that there is actually less use in the days from Dec-June when monitoring did not take place compared to the 29 days when monitoring occurred? If not, then perhaps the way to describe this assumption would be as a reasonable estimate, but not a conservative estimate. Overall, EPA is concerned that many of the assumptions used in this ERA described as "conservative" are actually not conservative. They may be reasonable estimates but not conservative. This leads to the ERA claiming that the resulting risk estimates are conservative estimates when in fact they are not. EPAs concern is that this in turn will lead to the RI/FS proposing cleanup options and describing them as conservative when in fact they are not, so then when it comes time to select a remedy, there will be pressure not to be overly "conservative" and to pick a more "reasonable" alternative that will actually be less protective than is warranted. **Response:** Comment addressed. The word "conservative" was removed from the discussion of the harbor seal site use factor.
71.	190	A.5.1.3.1	Please clarify what is meant by "whole-body tissue data" for crab tissue. In particular, please explain if this does this include shells **Response:** Comment addressed. Clarifying text was added to the footnote.
72.	198	A.5.1.3.3	Table 5-6: *"Uncertainties associated with the use of PCB TEQ instead of the total TEQ, which was not available, are expected to be low and are discussed in the uncertainty analyses for each ROC in Section 6.3."* It is inappropriate to drop dioxins & furans from consideration as COCs, both by themselves and as a component of total TEQ, without obtaining further data to assess the ecological risk. At the nearby Lockheed West site, there was a similar shortage of data on dioxins and furans, but these chemicals were identified as COCs based on their assumed presence in the sediments and seafood, and based on assumed cancer risk estimates above regulatory thresholds. Until data can be obtained to determine that they do not pose a risk, it is reasonable to assume that they do, based on their presence at adjacent sites. **Response:** Comment was withdrawn by EPA (meeting on July 21, 2011).. Dioxin/furans were evaluated for wildlife receptors based on dietary and incidental sediment ingestion. In addition, the exposures from ingestion of water are a very small component of the overall dose to wildlife receptors.

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73.	219	A.6	We recommend including a more complete description of interactions between contaminants to the uncertainty discussions. The decision to rely solely on laboratory studies of individual contaminants excludes field studies, which represent an important body of scientific literature with relevant (and possibly more realistic) environmental exposures. An assumption of additivity is the standard default for consideration of chemical mixtures in risk assessment. The uncertainty discussion must cite Nakayama et al 2008 or other mixture papers from the literature. The uncertainty discussion must address the issues identified above and state that the presence of chemical mixtures will tend to result in the actual risks being higher than predicted by the chemical-by-chemical risk assessment: interactions between chemicals with similar modes of action are likely to be additive. **Response:** Comment addressed. Text was added to uncertainty sections for benthic invertebrates (tissue-residue approach); crab (tissue residue approach), and for all fish and wildlife ROCs.
74.	220	A.6.1	Please explain why the risk for crabs is based on water but not sediment. **Response:** Comment addressed. This comment was addressed in the Problem Formulation, which presents the ERA approach. A footnote was added to Table A.2-15.
75.	220	A.6.1	Dredged Material Management Program (DMMP) guidelines are available for TBT and dioxins. Please revise. **Response:** The DMMP guidelines for TBT and dioxins are not based on whole sediment toxicity data and therefore were not used to characterize risks in the ERA.
76.	220	A.6.1.1.1	This definition of the CSL is incorrect. The correct definition is that the CSL is the level above which minor adverse effects to the benthic community are expected, not "may occur." *WAC 173-204-520, http://www.ecy.wa.gov/programs/tcp/regs/2009MTCA/issues/smsIssuePapersGlossaryJune2009.pdf*. Check the precise definition with Dept of Ecology and correct this definition each time it appears throughout the document (e.g. pages 123, 229). It appears correctly on page 125. **Response:** Comment addressed. Edits were made to the 2nd paragraph in Section A.3.2.1.1, the 1st paragraph in Section A.6.1.1.1, and the 1st paragraph after Table A.6-2.
77.	222	A.6.1.1.1	This description of COCs and COPCs is somewhat confusing because they appear to have the same definition in this case (for benthos in surface sediment, chemicals that exceed the SQS in at least one surface sediment sample, see table 2-15 page 54). Please clarify if these are intended to have different definitions, or if in this case, all COPCs are COCs. Also add COC to the list of acronyms at the beginning of the document. **Response:** Comment addressed. For benthic invertebrates, the criteria set for COC designation happen to result in the COPCs becoming COCs. One is based on maximum and other is any location exceeding SQS. The end result is the same. A footnote was added to clarify this.Text and acronym list was revised.
78.	230	A.6.1.1.1	The DMMP has issued interim guidelines for dioxins on December 6, 2010. The DMMP has also issued guidelines for TBT in its DMMP manual. At a minimum, these guidelines must be discussed with regards to the sampling data provided in the surface sediment data report. **Response:** The available DMMP guidelines for TBT and dioxins is discussed in a footnote added to the first paragraph of Section ES.3 and in text and footnote added to the third paragraph of Section 2.5.1.1. As noted in these text additions, these guidelines were not used in the ERA because either a different risk approach was used (i.e., for TBT a tissue residue approach used because no sediment SMS or DMMP value) or because the guideline was not related to benthic risk analysis (i.e., dioxin).
79.	232	A.6.1.1.1	The paragraph on reporting limit exceedances must state clearly what was done with the 25 locations that had only RL-based exceedances and no detected exceedances. Please explain if these were classified as exceedances or not. **Response:** Comment addressed. A clarifying sentence was added to the paragraph preceding Table A.6-3.

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80.	241	A.6.1.1.3	It is not clear why sometimes the terms NOEC and LOEC are used, and sometimes NOAEL and LOAEL. Please be consistent unless these terms are being quoted from studies. **Response:** Comment addressed. NOEC and LOEC are used in the ERA for water toxicity data because they are the more commonly used terms for water effects in the scientific literature. A footnote was added to the 3rd paragraph in section A.3.2.3.1, the first time the terms are used in the document.
81.	245	A.6.1.1.5	In the bulleted list, the first bullet should be edited to specify "In 40.4% of the EW, no adverse effects *from exposure to contaminated surface sediments* were predicted for benthic invertebrates living in intertidal and subtidal sediment." In other words, these areas were found to have sediment chem./tox results below the SQS, but that doesn't mean that there is no risk from the other pathways (porewater, surface water). Similar edits should be made for the 2nd and 3rd bullets, indicating that this risk characterization is only based on the sediment pathway. As written, the sentence preceding the bullet lists specifies that the bullet list is talking about the sediment-related risk, but bullet #4 includes the other pathways and the section heading indicates that this should be a summary of all the risk conclusions for benthic invertebrates. **Response:**. The requested edits were generally made to the bulleted list. However, porewater is present as a component of whole sediment samples that were used in the development of sediment AETs and therefore, the assessment of surface sediment includes the porewater associated with that sediment .
82.	246	A.6.1.1.5	Table 6-10: It is not clear what is meant by including copper in this table without an x in any of the columns. Explain whether copper is a COC for benthos. **Response:** Comment addressed. Copper is not a COC for the benthic invertebrate community; the row for copper was deleted from Table 6-10.
83.	248	A.6.1.2.1	first full sentence- This sentence should read "*effects* are considered unlikely" as opposed to risks. **Response:** Comment addressed. Edit was made.
84.	249	A.6.1.2.1	Exposure Concentrations, top paragraph: "The only portion of the crab that was not analyzed was the shell, which would not be expected to accumulate contaminants, so the exclusion of the shell would not be expected to result in an overestimation of the whole-body concentration including shell, which results in an overestimation of exposure and possibly risk." Some metal concentrations may be higher in the exoskeleton. This statement must be revised to reflect this uncertainty. **Response:** Comment addressed. Text was removed.

85.	249	A.6.1.2.1	Explain why excluding the shell would overestimate exposure and risk. A large proportion of the references are incorrectly cited in Attachment 3- (see subsequent comment on Attachment 3) that made it difficult to verify. For the ones where the citations are correct, they universally contradict this statement: a. Rice et al 1989 analyzed *tissues*, thus comparing *edible meat* concentrations to their data would not be an overestimate of risk. b. The statement that "the shell would not be expected to accumulate contaminants" directly contradicts Jennings and Rainbow 1979 for crab and cadmium. The abstract of that paper contains the following: "The highest tissue concentration was found in the midgut gland... The midgut gland only contained about 10% of the total cadmium absorbed from solution, while the exoskeleton contained the bulk of absorbed cadmium (59 to 80%) probably passively adsorbed onto the surface." c. This statement also contradicts Mortimer and Miller 1994 which states on pages 1117-1118: "Cr(V1) taken up by the animal is accumulated to higher concentrations in the exoskeleton than in the other tissues" and "Cr is preferentially sequestered in the exoskeleton". The literature cited in the ERA directly contradicts the claims made in the ERA. This claim must be deleted or else heavily modified. If the claim is that contamination accumulated on the shell would be a lesser hazard to the organism than contamination accumulated in internal organs, or if this claim was intended not to apply to inorganic contaminants, state this explicitly and provide citations. Since 4 of the 5 COPCs are inorganic, the behavior of inorganic contaminants cannot be overlooked. In general, there are multiple locations in the document where similar imprecise wording might lead to misinterpretations of risk by implying that a claim that relates only to one contaminant, receptor, or pathway applies to others as well. Editors of the ERA must scrupulously check to ensure that all claims made in the ERA are clearly stated with the appropriate qualifications and caveats. **Response:** Comment addressed. Text was removed.
86.	250	A.6.1.2.1	Table 6-13: Since TRVs for As and PCBs both have significant uncertainty, and since both exceed the NOAEL and are within a factor of 4 (As) and 2.5 (PCBs) of the LOAEL, more discussion with EPA is needed regarding retaining them as COCs. As stated in the document, TRVs for As and PCBs have high uncertainty because they are based only on survival data and not on potentially more sensitive endpoints such as growth and reproduction. Also, for As, all the TRVs are based on species other than crab. More data on more sensitive endpoints could potentially reduce the LOAELs for As and PCBs by a sufficient amount to bring the HQ above 1. **Response:** This comment was discussed with EPA on July 21, 2011, it was agreed that arsenic and PCBs would not be designated as COCs for crab. Additional text was added to acknowledge uncertainties in risk analysis.
87.	250	A.6.1.2.1	(TRVs – Crab): Please provide the reference (Jennings and Rainbow 1979) in the text discussing comparison of TRVs from this study to edible mean concentrations of cadmium. **Response:** Comment addressed. The Jennings and Rainbow reference was added to the text.
88.	254	A.6.2.1	NOAELs and LOAELs are not bright line thresholds since the dosing studies spread their doses out over a range of concentrations and may not represent the actual threshold. Therefore concentrations equal to or greater than a NOAEL potentially puts juvenile salmon at risk. Please use greater than *or equal to*, not just greater than the NOAEL to flag risk for salmon. Juvenile Salmon require more conservatism than the non-listed species. **Response:** The identification of COCs for juvenile Chinook based on exposure concentrations above the NOAEL rather than the LOAEL is conservative. The NOAEL is the concentration associated with no effect on the receptor and therefore concentrations equal to the NOAEL are not associated with effects or risk. The treatment of threatened species in this ERA is consistent with the ERA conducted for the LDW.

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89.	255	A.6.2.1.1	Table 6-16: Juvenile Chinook salmon may be at risk from arsenic in diet. As a listed species they are protected at the individual level. Since the 95% UCL for arsenic in benthic invertebrate tissue is equal to the NOAEL, there is the potential to adversely impact this species on an individual level. Therefore, please bold the value and change the footnote for arsenic to be greater than *or equal to* the NOAEL. Additionally, if the 95% UCL is less than the maximum arsenic concentration in tissue, please use the maximum detected concentration for comparison to the TRV to be protective at the level of the individual. Response: The identification of COCs for juvenile Chinook based on exposure concentrations above the NOAEL rather than the LOAEL is conservative. The NOAEL is the concentration associated with no effect on the receptor and therefore concentrations equal to the NOAEL are not associated with effects or risk. The treatment of threatened species in this ERA is consistent with the ERA conducted for the LDW.. The 95th UCL on the diet tissue is the EPC which is most appropriate for the diet of individual fish as well as fish populations.
90.	257	A.6.2.1.2	Text downplays the importance of benthic species in juvenile Chinook diets. Please revise accordingly. **Response:** Comment addressed. Text was revised.
91.	258	A.6.2.1.2	Table 6-19: Rationale for Cd in the table – remove the speculation that food avoidance resulted in the effect since no data support this speculation. Stating that the LOAEL was much lower than the other studies is sufficient explanation and the only statement supported by data. Rationale for As in the table, the low uncertainty is supported by a statement that gives weight to the use of salmonid species. This contradicts what is said about salmonids for cadmium on the next very page, see comments for Pages 259 and 262. **Response:** Comment addressed. Revisions were made to Table 6-19.
92.	258	A.6.2.1.2	Text need to clearly state lack of data on multiple chemical exposures when limited to laboratory effects data vs. a combination of laboratory and field studies. **Response:** Comment addressed. Text was revised.
93.	259	A.6.2.1.2	last paragraph. It is not appropriate to include the study by Handy 1993, where the author states that the fish expelled the ingested food making the received dose *unknown. Please revise the text accordingly.* **Response:** Comment addressed. The Handy study was removed from Table A.6-20 and text was revised.
94.	260	A.6.2.1.2	Please explain if the conclusion regarding copper considers behavioral endpoints. **Response:** Comment addressed. Text was added to the introductory paragraph of Section A.6.2.1 stating that risk was evaluated using growth, survival, and reproductive endpoints.
95.	265	A.6.2.2.1	(Risk Estimates- English sole): Revise TBT TRV (as directed in earlier comment) and HQ presented in Table A.6-24 as well as text discussion (pg 266). **Response:** Comment addressed. Requested revisions were made.
96.	267	A.6.2.2.2	The version of this table that appears in the February 25 draft inadvertently switched the columns for 0% and 1% sediment ingestion. Please correct this error. **Response:** Comment addressed. Table A.6-25 was corrected.

97.	269	A.6.2.2.2	The 3 detailed pages (269 – 272) describing uncertainty for a single paper completely misrepresents the amount of uncertainties associated with the Hugla and Thome (1999) paper. All the reviewed tissue residue papers have uncertainty, and many have much more uncertainty than this paper. Including so much detail for only 1 paper misrepresents the literature for PCB TRVs. For example, only 1 short paragraph is dedicated to describing the uncertainties with the Hansen et al. (1974) paper. Many uncertainties are not included and not discussed in detail for that paper. Please remove or significantly shorten the section devoted to one paper, or give all papers equal critiques for a balanced perspective. National expert scientists previously reviewed Hugla and Thome (1999) and independently concluded that the data quality was above average for this type of study. Please remove or significantly shorten this text and make it comparable to other such discussions of uncertainty. **Response:** EWG recognizes EPA's concerns with the lengthy text regarding the Hugla and Thome (1999) paper. However, as part of the LDW ERA, substantial effort was made to come to an agreement between EPA and LDWG on the content of Hugla and Thome description. Since no new information has arisen since this text was finalized in the LDW ERA in 2007 the text will not be revised.
98.	272	A.6.2.2.2	A.6.2.2.2 (Uncertainty Analysis): Text refers to data from Fisher et al. 1994 reference that does not appear in Table A.4-13. **Response:** Comment addressed. Fisher et al. 1994 was added to Table A.4-13.
99.	272	A.6.2.2.2	2nd paragraph. The statement citing Niimi (1983) for estimating concentrations related to changes in egg mass with water uptake is not supported by any data in that paper, and his conjecture is incorrect based on data in more recent work. Measured water uptake is much less than 2 times and has been measured to be about 30% (Lahnsteiner et al. 1999) or as little as 8% in rainbow trout depending on the age and quality of the egg (Lahnsteiner et al. 2000). Therefore egg concentrations reported in this paragraph need to be revised consistent with this literature. Please make this correction. Early-life stages are typically the most sensitive period to toxicity from many contaminants, so effects thresholds are much lower (order of magnitude). Using lipid-normalized concentrations is the best way to apply early life stage TRVs to adult life stages (Russell et al. 1999). **Response:** Comment addressed. The information on water uptake in fertilized eggs was removed from this discussion and calculations were revised accordingly.
100.	273	A.6.2.2.2	2nd to last bullet. While the Hugla and Thome (1999) paper has already been discussed extensively, it is important for this Ecological Risk Assessment to present accurate information and an accurate and balanced assessment of uncertainties. The HQ for PCBs from that paper is between 1.6 and 7.9. The higher LOAEL that results in a HQ of 1.6 results from an endpoint that reflects a complete absence of spawning, so to imply that this is the better HQ is incorrect, since the effect and impact were extremely severe. In addition, many important endpoints were not included for PCBs, so the TRV is not based on a complete dataset of important PCB effects. Altered behavior and alterations in immunity have important consequences for survival, and these endpoints with lower TRVs were not included. Therefore, please revise this paragraph to include an unbiased treatment of all the important uncertainties such as the following: "The HQ calculation for PCBs came from a single paper and ranged from 1.6 to 7.9. The PCB HQ likely falls within that range with uncertainty surrounding the higher HQ (based on lowest LOAEL fecundity endpoint). The severe effects reported at the lower HQ (highest LOAEL – no spawning) indicate that the HQ is likely to be greater than 1.6 based on this data. However, other potentially important effect endpoints from other papers were not included; therefore the bias in the HQ range is uncertain." **Response:** The text discussing risk uncertainties for the two different HQs based on Hugla and Thome have been removed.
101.	276	A.6.2.3.1	(Risk Estimates- Rockfish): Revise TBT and Mercury TRVs (as directed in earlier comments) and HQs presented in Tables A.6-28 and A.6-29 as well as text discussion (particularly related to the mercury HQ derived using individual rockfish tissue data). **Response:** Comment addressed. Tables A.6-28 and A.6-29 and associated text were revised accordingly.

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102.	284	A.6.2.3.3	(Risk conclusions): Revise Table A.6-33 to reflect revised Fish HQs for TBT and Mercury. **Response:** Comment addressed. Table A.6-33 was revised.
103.	284	A.6.2.3.3	Table 6-33: Mercury content in brown rockfish is within Hg residue-effects range for fish (see Beckvar et al. 2005 and Dillon et al. 2010); therefore, more discussion is needed regarding the selection of mercury as a COC for this receptor. **Response:** Given that the site-wide EPC is below the fish TRV for Hg (0.39 ppm wet), EPA has determined that Hg is not a COC for rockfish.
104.	285	A.6.2.3.3	(Risk conclusions): Add text discussing the mercury HQ's derived for individual rockfish and ramifications to risk conclusions for this ROC. **Response:** Given that the site-wide EPC is below the fish TRV for Hg (0.39 ppm wet), EPA has determined that Hg is not a COC for rockfish.
105.	307	A.6.3.1	Table A.6-48: Chemicals identified as COCs for wildlife ROCs, this table has typos, "ne?". Please proofread and correct. **Response:** Comment addressed. The abbreviation "ne" in this table means "not evaluated" as identified in the acronym list at the bottom of the table.
106.	308	A.7	More discussion on selection of Ecological Risk drivers is needed between EPA and the EWG. **Response:** Risk drivers have been retained in Section A.7. Additional language has been added to Section 7 detailing the importance of both risk drivers and COCs in the SRI and FS. The identification of risk drivers in the EW ERA is consistent with the identification of risk drivers in the LDW ERA.
107.	308	A.7	More discussion is needed with EPA regarding how the selection of ecological risk drivers interacts with the FS and what happens to the predicted risk that are not drivers. There is a similar section in the Duwamish risk assessment that may be useful, however EPA is concerned about how it diminishes predicted risk. **Response:** Risk drivers have been retained in Section A.7. Additional language has been added to Section 7 detailing the importance of both risk drivers and COCs in the SRI and FS. The identification of risk drivers in the EW ERA is consistent with the identification of risk drivers in the LDW ERA. The extent to which the remediation of risk drivers reduces the risk associated with COCs will be evaluated in the FS.
108.	308	A.7.1	TBT will be carried forward as a risk driver for benthos when based on the revised 0.12 mg/kg ww TBT TRV **Response:** Comment addressed. TBT has been identified as a risk driver for the benthic invertebrate community.
109.	308	A.7.2	TBT will be carried forward as a risk driver for fish based on applying the 0.290 ppm wet TRV to both EPC (population) and individual rock fish residues. Please add this to this section. **Response:** The EPC for rockfish at the population level does not exceed the TRV, even though three individual fish slightly exceed the TRV with HQs of 1.1, 1.1, and 1.8. Brown rockfish are not threatened or endangered and therefore while TBT is a COC for rockfish, it is not a risk driver for rockfish.
110.	313	A.7.2	(Risk Driver Evaluation for Fish): Add data for mercury HQs for individual Rockfish to Table A.7-2 and text discussion. **Response:** Comment withdrawn in comment clarification from EPA (email dated August 16, 2011).
111.	317	A.8.2	(Summary of Risk Driver Chemicals): Add mercury to the COCs (PCBs and TBT) listed for Fish based on tissue residue based on HQ> 1 based on comparisons using individual rockfish data.

			Response: Comment withdrawn in comment clarification from EPA (email dated August 16, 2011).
112.	1	Attachment 2	Please correct C9 on page 1 of Attachment 2 (C9 currently repeats C5 but must read "Chemical was detected in at least one porewater sample"). **Response:** Comment addressed. Correction was made.
113.	3	Attachment 2	Table 1 in Attachment 2 must differentiate between na (not analyzed, which is appropriate for C2, C3, C5, C6, C7, C8, and C9) and no (which is appropriate for C1 and C4, as well as for nondetects in the other criteria). This is generally done correctly, but there are some instances where na is listed for C1, which causes reviewers to question whether na and no are being used interchangeably in the other columns where they have different meanings. Please review and revise accordingly. **Response:** Comment addressed. Table was revised so that na does not appear in columns for C1 or C4.
114.	15	Attachment 2	Table 2 in Attachment 2: error in the Mercury row under Fish Tissue- it appears to be in Wingdings font or have some typographical error. Please correct. **Response:** Comment addressed. Error was corrected.
115.	15	Attachment 2	Table 2 in Attachment 2: na shows up in the table for vanadium and zinc but not in the key at the end. Is this an error that should read either no or ne? Please clarify. **Response:** Comment addressed. Incorrect entries in the fish columns were corrected and footnote for ne was revised.
116.	4	Attachment 3	Attachment 3: Table 2 cites Duke et al 1974 but no such reference is listed. Is this Duke et al 1970? Please correct the reference. Same comment for the references to: Weis 1986b; Weimin et al 1978; Neff and Giam 1974; Hansen et al 1977; Duke et al 1972; Sanders and Chandler 1974; Canli and Furness 1982; Dickson et al 1995; Gillespie et al 1979; Thorp et al 1980; Pesch and Stewart 1994; Vernberg et al 1996; Rule and Alden 1982. Some of the incorrect dates are probably a result of merging author and date columns, which is an understandable error, but needs to be corrected in the next version of the ERA. However, some entries (such as Vernberg) seem to be missing altogether from the list of references. Please correct. **Response:** Comment addressed. References were corrected. However, they were not corrected in redline because this using redline with the Endnote software causes instability in Word documents.

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